On the enfeeblement of mathematical skills by 'Modern Mathematics' and by similar soft intellectual trash in schools and universities*

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§1.

The word 'modern' comes from the Latin *modo*, meaning just now, here today, gone tomorrow, ephemeral. More often that not we speak in this sense of modern art. Here is the artist setting aside established traditional skills and having instead his contemporary fling: put the canvas on the floor and the paint on your bicycle wheels, ride around, and hope that someone else's psyche will make something of the result. From all such entirely legitimate experiment only a tiny fraction will survive as an original and significant addition to human achievement, and the rest will be forgotten as all yesterday's nonentities and trivia always have been. Collectors know how hard it is to spot tomorrow's gems in today's midden. On the other hand, speaking of modern languages we mean as a rule living languages, words and expressions which men use in their daily affairs for their current thoughts and emotions, for chatter, for diplomacy, for professional and technical communication, and for commercial traffickings. That contrasts a modern with a dead language. The latter has its splendours and is not obsolete; but it is out of date and, as any encaenia shows only too plainly, it lacks the terminology and range of expression to handle many of the concepts that mankind has now come to hold important or to reach towards. I want to consider later whether modern mathematics is modern in the sense of modern art or of modern languages.

In 1932 T. S. Eliot, writing on *Modern education and the classics* (ref.(1)), said this:

'Questions of education are frequently discussed as if they bore no relation to the social system in which and for which the education is carried on. This is one of the commonest reasons for the unsatisfactoriness of the answers . . . our questions raise other questions, social, economic, financial, political. And the bearings are on more ultimate problems even than these: to know what we want in education we must know what we want in general . . . The progress (I do not mean the extension) of education for several centuries . . . has tended to be dominated by the idea of *getting on*. The individual wants more education, not as an aid to the acquisition of wisdom but in order to get on; the nation wants more in order to get the better of other nations, the class wants it to get the better of other classes, or at least to hold its own against them. Education is associated therefore with technical efficiency on the one hand, and with rising in society on the other. Education becomes something to which everybody has a "right", even irrespective of his capacity; and when everyone gets it—by that time, of course, in a diluted and adulterated form—then we naturally discover that education is no longer an infallible means of getting on, and people turn to another fallacy: that of "education for leisure"—without having revised their notions of "leisure". As soon as this precious motive of snobbery evaporates, the zest has gone out of education; if it is not going to mean more money, or more power over others, or a better social position, or at least a steady and respectable job, few people are going to take the trouble to acquire education. For deteriorate it as you may, education is still going to demand a good deal of drudgery. And the majority of people are incapable of enjoying leisure . . . in any but pretty simple forms—such as balls propelled . . . The uneducated man with an empty mind if he be free from financial anxiety or narrow limitation, and can obtain access to golf-clubs, dance halls, etc., is, for all I can see, as well equipped to fill his leisure contentedly as is the educated man'.

These words of Eliot's ring true and seem more relevant now than when he wrote them a generation ago.

I know it is rash of me to have included schools in the title of this lecture, for I have not taught in schools. I hope therefore that what I say about schools will be treated only as initiating a discussion, to which schoolteachers themselves will contribute from their own authoritative standpoints and so correct any of my misapprehensions. My reason for including schools is my impression that some schoolteachers accept modern mathematics for the schools, not for any inherent merits which they themselves perceive in it, but because they have been led to understand that it is highly regarded by university mathematicians who (so the argument runs) ought to know best. I wish to counter this line of thought by stating quite categorically that not all university mathematicians approve of modern mathematics and that, even if they did, they are in no special position to know best what is suitable for schools. Certainly an influential subsection of university mathematicians do support modern mathematics; but this subsection consists mainly of pure mathematicians with little direct personal experience of using mathematics for practical ends. There is no reason to suppose them specially qualified to pronounce on school syllabuses or to discern which types of mathematical skill are most important for industry, or engineering, or scientific research, or for the more general needs of society as a whole. Only a minority of school leavers go on to tertiary education, and only a fraction of this minority will specialise in mathe-

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matics at the university. If there is a case for introducing modern mathematics into the schoolroom, that case must rest upon arguments and evidence adduced in the schools themselves and upon the general ends which a school education seeks to meet for society as a whole, rather than upon any supposedly higher authority of mathematical specialists in the universities.

But my concern for schoolteaching has been fortified by letters sent me since the announcement of the title of this lecture. It was not, I suppose, to be expected that many who disagree with my apparent thesis would write to me, and indeed none of these letters seek to defend modern mathematics. It is not the unanimity of the correspondence which reassures me: what does reassure me is that many of these letters come from people with very considerable experience of schoolteaching. I do not propose to summarise the various points made in this correspondence, because it will be far better for schoolteachers (representing both sides of the argument) to contribute to the discussion themselves. But I should like to make an exception for a letter from Mr. Sydney Adams, for the personal reason that he taught me my mathematics when I was a schoolboy. Amongst other things he has this to say:

> '... Fortyish years of trying have convinced me of two things: one, that there are not enough good mathematicians in the schools and two, that the pursuit of a truly "numerate" democracy is a striving after the wind. The number of people with real mathematical insight is strictly limited. My fear is that we shall turn out from the schools a generation with the "patter" and no real understanding of the ideas. My generation acquired the "patter" of psycho-analysis—*The A.B.C. of Psycho-analysis*, price one shilling and sixpence—and our real knowledge was nil . . . I like the Miltonic rotundity of the title of your lecture and believe it to imply something which needs to be said. I have been talking to some of my seniors and their—perhaps over contemptuous—assessment of what they have heard about "Modern Mathematics" was O-level without tears or even without work . . .'

I do not know whether schoolchildren, who have undergone a more systematic exposure to modern mathematics than Mr. Adams' senior pupils, would endorse this view that modern mathematics is contemptibly trivial; but it would be a very serious matter if they did, for it would lead them to equate mathematics with baseless pretension. If education is to command respect, it must be challenging. I have tried to reach a personal conclusion on this question by comparing current G.C.E. examination papers in traditional and modern mathematics. These leave me in no doubt that the traditional syllabus demands greater achievement as far as examination standards go. Of course, when examinations are first set on any new syllabus, there is a natural reluctance to pitch the level too high lest schoolteachers be frightened off. But it may also be the case that the proponents of modern mathematics or new syllabuses, in trying to realise a numerate democracy, have had to lower standards and dilute the mathematical content to meet the necessarily slighter attainments of their enlarged clientele. It would be a great pity if this lets the abler children default on achieving their full potential, for the country needs all their talents.

§2.

A serious weakness in modern mathematics is its preoccupation with mathematical jargon and abstract mathematical structure, which foster the patter mentioned above. Professor D. B. Scott has called my attention to the following definition:

> '*Ju-ju*: that branch of science in which, by giving names to things, we thereby acquire power over them'.

There is more in this definition than first meets the eye. People do acquire a little brief authority by equipping themselves with jargon: they can pontificate and air a superficial expertise. But what we should ask of the educated mathematician is not what he can speechify about, nor even what he knows about the existing corpus of mathematical knowledge, but rather what can he now do with his learning and whether he can actually solve mathematical problems arising in practice. In short, we look for deeds not words.

There are three topics which inculcate jargon if introduced into the school syllabus:

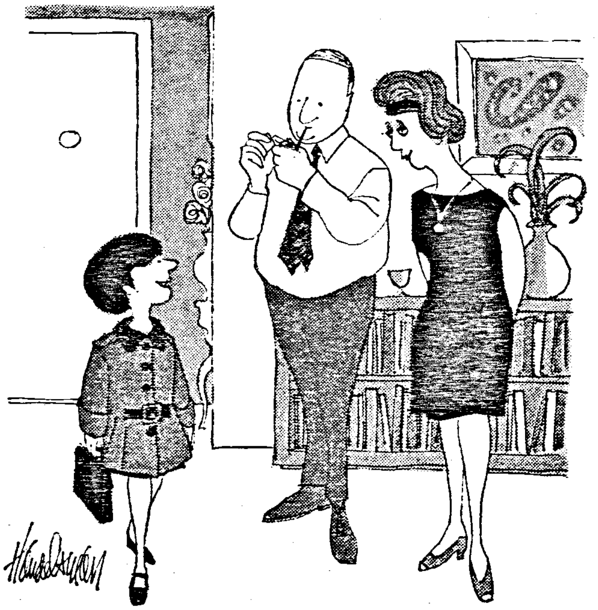
(i) *Set Theory*. At the elementary level this is a dull subject with no worthwhile theorems. Of course, I cannot object to the use of set-theoretic language and symbolism, provided it is employed in a perfectly natural and unemphatic manner without clogging up manipulations (see Appendix I); but we should not delude ourselves that this amounts to mathematical reasoning.

(ii) *Definitions and foundations of the real number system*. This has its points for mathematical undergraduates, but it is out of place in the schoolroom. You will not make a schoolchild into a better prospective engineer by teaching him that a fraction is an equivalence class of ordered pairs of integers: you will merely occupy time that might have been better spent on introducing him to some elementary calculus, say, or to concepts like momentum, energy, magnetic fields, and so on. I do not believe that British school programmes of modern mathematics dwell on the real number system at any great length; but it certainly disfigures several of the American programmes.

(iii) *Abstract algebra and vector spaces*. In moderation this is acceptable; for instance, an early introduction to the use of matrices is fine. But it can very easily be overdone, especially if the emphasis is on algebraic structure rather than on manipulation and applications.

These are not the only school topics where jargon and verbiage are too rife in modern syllabuses, but they are the main offenders.

It is often said that a student will most readily learn how to utilize some particular mathematical technique in a practical situation if he first understands the underlying theory. This dictum might be valid if his understanding of the theory were complete and readily gained. The sad fact is that most students do not fully understand the theory; or, if their theoretical understanding becomes more or less complete, they have usually gained it with such effort that they have little time or mental energy left to pursue applications. Whilst this alas applies to most students, there remains yet a minority of brighter pupils who absorb theory readily; but the larger part of this minority is overtaken by another fate—they enjoy the theory so much that they become professors of pure



"I'm not learning anything. I'm developing cognitive skills."

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mathematics and so have little occasion, let alone inclination, to apply their art.

To exemplify this, mathematical undergraduates at Oxford in their first two years study algebra (*inter alia*) from a theoretical viewpoint. The following question, taken from the 1965 Final Honours examination, typifies what they are expected to know about vector spaces:

'Show that in any Euclidean vector space V (i.e. a finite dimensional vector space over the real field endowed with a positive definite symmetric inner product) a linear transformation α: $V \rightarrow V$ is a perpendicular projection if and only if $\alpha = \alpha^2 = \alpha^*$, where α^* denotes the transformation adjoint to α. Vectors in V are represented relative to an orthonormal basis by n-tuples, and linear transformations of V are represented by square matrices acting on column vectors. If W is a subspace of V spanned by m linearly independent vectors, represented by column matrices $x_1, \ldots, x_m$, and X is the $n \times m$ matrix $(x_1, \ldots, x_m)$, show that the matrix of the perpendicular projection of V on W is $X(X'X)^{-1}X'$, where X' is the transpose of X. Hence, or otherwise, find the matrix of the perpendicular projection of a four-dimensional space on the subspace spanned by the vectors (1, 0, 2, −1) and (0, 1, 1, −1)'.

To such a question many undergraduates will give a two-page answer filled with a good deal of periphrasis about inner products and orthonormal bases. But the question is really very simple, almost trivial. Start with the second part of the question, and let T be the matrix of the projection. Then $TX = X$, since the projection leaves points of W fixed. It also annihilates any vector x orthogonal to W, so $X'x = 0$ implies $Tx = 0$. Geometry guarantees that T is the unique matrix with these two properties; $X'X$ is obviously positive-definite and so non-singular; and now subsitution shows that $T = X(X'X)^{-1}X'$ meets the requirements. From this, the first part of the question (which becomes $T = T^2 = T'$ when written in decent language) follows easily enough; and the third part of the question is easy arithmetic. My point is not so much that students are thus schooled to puff theoretical hot air, but rather that this schooling does not help them put this knowledge to use. When they come to do statistics in their third year and have to manipulate matrices of orthogonal projections for least squares and regression, they find it difficult at first: often not until they have done practical applications like this do they come to see what the theory is all about. I speak with feeling, having delivered third-year lectures on statistics.

These remarks will not surprise schoolteachers. They have to cover the whole syllabus. Customarily they use examples and applications for driving home the theory. The canard that abstract theory is a helpful preliminary to applied mathematics is more rife amongst university mathematicians, who may stick to pure mathematics while leaving applications to a colleague.

There is also a belief in some quarters that mathematics is a dendritic discipline: that you must proceed sequentially from the root to the branches, that no topic can be tasted before digesting its antecedents; that no theorem should be stated or used unless the student can master its proof. Even if such a doctrine is met on the score of logic (and this is arguable), it is didactically bad. No single failure of comprehension should be an impasse to all further progress; for there are many by-ways to understanding. Far too much stress can be laid on learning a subject 'properly', according to accepted canons of the day. Some years ago a protagonist of mathematical reform in the United States was explaining to me his approach to geometry from the axiomatic end. Euclid, he said, had done a job which, while admirable enough for the Greeks, was really rather rough and ready. Hilbert had seen some of the flaws in Euclid and had gone somewhat towards rectifying them. But the new American way had at last perfected things and had finally settled the 'proper' presentation of the axioms of elementary geometry. I could only reflect to myself that Euclid and Hilbert were greater mathematicians than he. The history of mathematics is full of subsequently discredited arguments by the greatest mathematicians. I doubt if there is any such thing as absolute mathematical proof: the best one can hope for is an argument that will satisfy and convince contemporary referees. The only final settlement is oblivion.

§3.

I mentioned earlier my belief that standards, as revealed by G.C.E. examinations, were lower in the new syllabuses than in the traditional ones. I examine this point in more detail in Appendix II, with reference to some papers set for the School Mathematics Project. Perhaps I am guilty of devoting too much attention to the S.M.P. examinations. The School Mathematics Project is not the only scheme for giving the school syllabus a new look: and some of the other schemes are more sharply attuned to 'modern' mathematics. However, I felt it better to comment upon one scheme in some detail rather than make unspecific and general comments upon a variety of schemes; and, in selecting one scheme for that purpose it was worth choosing the one that has had most prominence and success. Moreover, although my criticism has tended to be adverse in the present context, there are undoubtedly many good features in the School Mathe-

matics Project and I am well aware that their textbooks have been written with great care and devotion by some really excellent schoolteachers. For their successes, and for all their hard work, they have rightly been praised and I should in no way wish to detract from that. You may think too that a scheme is to be judged less on its examinations than on what it conveys in the classroom. However, a consumer of mathematicians is concerned with the results and not the means of education. Classroom methods are of no importance to him except in so far as they lead to an acceptable quality in the end-product; and examinations, despite their shortcomings, are the best indicators so far devised for assessing that quality. University teachers, as consumers of part of the school output, judge the the quality of that output in terms of G.C.E. results and scholarship examinations; and employers of graduates look at the performance in the Final Honours examinations. And all such consumers, when making judgements in terms of these examination results, are implicitly or explicitly concerned above all else with the contents and the standards of the examination papers themselves. Thus consumers are mainly unsympathetic to the fashion, which has been growing in political and even educational circles, of decrying examinations today, especially when the examinations are competitive and offer the chance of making comparisons. To the consumer an examination succeeds if it succeeds in discriminating. I readily concede that examinations are always in some respects imperfect; and I have myself set enough bad examination questions to know how easy it is to go awry despite the best will in the world. But I would stoutly maintain that examinations are important, especially for the better pupils. They provide concrete goals for achievement, they encourage hard work, and I do not like the spirit of egalitarianism which proclaims that none should excel because, by definition, all cannot. Let us not abolish the Olympic Games because so few of us are four-minute milers. I very much welcome the remarks on examinations in the 1966-67 Director's Report (2) of the School Mathematics Project:

'. . . if A-level examinations were declared to be school-leaving examinations, then those working on new curricula would be relieved of the necessity of constantly looking ahead to the tertiary level . . . First, it is clear that examiners, especially at A-level, are finding it increasingly difficult to set questions which accurately match *both* the changed content in the curriculum and the changed methodology in the classroom; once one abandons the well-worn traditional paths, one finds oneself remarkably soon in uncharted territory. Second, the more deeply an examiner ponders upon the character of his examinations, the more insistently is he faced with the fundamental question of what examinations in general are supposed to achieve . . . Many of these considerations must have been in the minds of the A-level examiners when, in their report on the 1967 examination, they recommended the setting up of a research team to set the 1969 examination. This recommendation has yet to be acted upon, but readers of this Report will be interested to hear that the S.M.P. regards the problem of examinations as one which requires a research effort comparable with that which has gone into the writing of the S.M.P. texts . . .'.

That is well said and well directed; and I am sure that everyone will wish success for these intentions—for indeed the vigour and faith of those who have worked so hard for the School Mathematics Project richly deserve and must surely attain what they seek.

To be sure, the worst excesses of modern mathematics occur abroad; and we may hope they never penetrate our own shores. As an example, consider the following extract on the solution of quadratic equations taken from *Synopses for Modern Secondary School Mathematics*(3), (p. 31) compiled by the Organization for European Economic Co-operation:

'Consider an equation such as $(x-1)\ (x+3)=0$. In order that this be satisfied, it is necessary, and this is sufficient, that one, at least, of the factors of the product $(x-1)\ (x+3)$ vanishes. We therefore have the equivalence

$$(x-1)\ (x+3)=0 \iff x-1=0 \text{ or } x+\ =0$$

Therefore, since $\{x;\ x-1=0\}=\{1\}$
and $\{x;\ x+3=0\}=\{3\}$
we have as the solution set of the equation $(x-1)\ (x+3)=0$

$$\{1\}\cup\{3\}=\{1,\ 3\}.\text{'}$$

How do the distinguished authors of this treatise arrive at the wrong answer? Is it merely a misprint? Doubtless $x+\ =0$ is a misprint; but later, when 3 occurs three times instead of -3, we begin to suspect an error in the original manuscript. And this is plausible in view of the cumbersome notation. It is a distraction to use the language of set theory for quadratic equations; and this point is not as trifling as it may at first appear. Any mathematical argument, even an elementary one, contains so many strands of thought that, if we peer too closely at each, we shall lose sight of the whole fabric. One of the prime purposes of notation and of manipulative technique is to relieve the mind of routine mechanical details. The early algebraists were as much hampered by their lack of an efficient notation as by anything else. Consider the following sentence from Cardan's discussion of the cubic equation (published in 1545 and subsequently translated(4) (Vol. I, p. 205) from the Latin by Professor McClenon): 'By the demonstration, the difference between 3 times *CB* times the square of *AC*, and 3 times *AC* times the square of *BC*, is [3 times] the product of *AB*, *BC*, and *AC*'. (Cardan forgot the factor in square brackets, and no wonder!). Nowadays we write $3vu^2-3uv^2=3(u-v)uv$, and need no separate demonstration for that. The authors of the O.E.E.C. treatise are plying a less efficient technique than the traditional one we already possess. They may claim their treatment leads to 'greater understanding'. Maybe; or maybe not. What it certainly leads to is an enfeebled mathematical skill. It is often more important to find the right solution to a problem than to understand the logic of the method. This is especially so for people who use mathematics as a tool in some other context. Similarly, one can well use an electronic computer without being obliged to understand its circuitry or even much about its software. There is force behind Bertrand Russell's aphorism that a mathematician never knows what he is talking about, nor whether what he is saying is true.

§4.

Education, you will surely recollect, has been defined as the casting of sham pearls before real swine. How sham

then is mathematical education: indeed what is *real* mathematics? Easily answered: ask any mathematician, and he will tell you at once that real mathematics is the mathematics he himself does. *Quot homines, tot sententiae*. So, when we talk about intellectual trash, we must remember that the subject has its own local topology: one man's trash is another pig's swill. And, as George Orwell said, all animals are equal . . .

It may help if we cast an eye over some of the ways of classifying the species in the mathematical menagerie. Mathematics may be pure or applied, abstract or concrete, theoretical or experimental, useful or useless, modern or traditional, hard or soft. These classifications are relative; a piece of work—for example, that on comets to be mentioned presently—may appear as applied mathematics to the mathematician and as theory to the astronomer. So there are no clear-cut boundaries, the extent of each territory lies somewhat in the eye of the beholder, they overlap and they are not independent. Yet, broadly speaking, we all recognize what these classifications comprise, even though the allocation of individual cases may be a matter of taste, temperament, and context. Therefore you will not expect me to define my terms. However, I should like to venture one or two remarks about hard and soft mathematics, because my central theme lies very much in that distinction. As I see it, the distinction between hard and soft is not that between difficult and easy, despite any positive correlation; instead it resides in a *commitment* to achieve prescribed goals, to solve a stated problem, and not to be diverted by the attractions of incidental generalities or circumambient atmosphere. Hard mathematics typically involves the focussing of interest and the marshalling of resources for a solution; soft mathematics the contemplation, the rearrangement, and the reinterpretation of the general panorama of what is already solved. There is a tendency for pure mathematics to be soft and applied mathematics hard; but it is only a tendency, and there are many exceptions—for example, number theory is mainly hard. Pólya is thinking of the soft mathematical aspect of pure mathematics when he typifies the pure mathematician as one who says 'Here is a problem: what shall we do with it? Throw it away and think of another one!'

Bernard Shaw said that those who can, do; those who can't, teach.* In mathematics, those who can't, expound; and what they expound is usually soft mathematics.

I have been reading again the symposium on modern mathematics published in the *Mathematical Gazette* in 1963, in particular the leading article(5) by Dr. Frank Smithies entitled 'What is Modern Mathematics?' I daresay that some of you will be longing for me to declare that it is intellectual trash; but that would be quite wrong or, even if right, beside the point. Dr. Smithies was specifically invited by the editor of the *Gazette* to answer the question in his title, and he has done so in a very lucid and masterly fashion, as indeed one would expect of a man of his wide knowledge and distinction. It is not his fault if anyone should feel unexcited by the subject matter: that would merely be a reflection on the reader's individual taste towards modern mathematics itself. We must accept that what Dr. Smithies describes is indeed the language and mode of thought of a considerable body of accomplished contemporary pure mathematicians. This fact we cannot escape. We cannot deny the existence of scholars of Anglo-Saxon and Old High German, or dispute their right to study these subjects to the limits of their fibre. On the other hand, we may reasonably enquire to what extent the man-in-the-street, whose wont and pleasure it is to read Shakespeare, needs to be first conversant with mediaeval literature. Just for the fun of getting his reactions, I asked an eminent scholar of English Literature what educational benefits might lie in the study of goliardic verse, Erse curses, and runic erotica.

'A working background of goliardic verse would be more than helpful to anyone hoping to have some modest facility in his own mother tongue', he declared; and with that he warmed to his subject and to the poverties of unlettered science, so that it was some minutes before I could steer him back to the Erse curses, about which he seemed a good deal less enthusiastic.

'Really', he said, 'that sort of thing isn't my subject at all. Of course, I applaud breadth of vocabulary; and you never know when some seemingly useless piece of knowledge may not turn out to be of cardinal practical importance. I could certainly *envisage* a situation in which they might come in very handy indeed'.

'And runic erotica?'

'Not extant'. Was it only my fancy that heard a note of faint regret in his reply?

Certainly the higher flights of scholarship can add savour; but does the man-in-the-street have the time and the pertinacity and the intellectual digestion for them? It is all a question of relative values and priorities. Whereas universities have stood, and still stand, for the scholar pursuing his own bent for the advancement of the study itself and his own delight in it, the modern world of education has encroached with public subventions and expectations of vocational training for large numbers of students within the framework of nationally conceived student/staff ratios. All this is very painful for the scholar who, set to teach, naturally inclines to teaching that which he himself knows and enjoys. Some of his pupils will be incipient scholars; but there will also be droves of freshmen, embarking upon mathematics for no better reason than its having been their best school subject. Like uncomplaining and powerless sheep they pass through the pens and paddocks of an academic syllabus. If the teacher wishes to impart to this flock some of his own enthusiasms for a living subject, is he wise to dwell too much upon the nicer (but more mortifying) points of structure, logic, syntax, and grammar? There is a heartfelt passage (6) (p. 38) in Lionel Hale's novel *A Fleece of Lambs*:

> 'It had never occurred to Sophia, nor to any of the other girls in the Latin class to connect the words on the printed page with anything that ever really happened. Men marched, camps were struck, winter quarters were gone into; but to Sophia the Latin language did not concern men, camps, winter quarters and cavalry. It existed to provide Subjunctives, and Past Participles, and (Oh golly!) Gerunds'.

In the introduction to his book *Fundamentals of Abstract Analysis*(7), which he calls 'a very abstract and highly formalistic book', Professor A. Gleason writes:

'It is unfortunate that the technical devices necessary

* And (as someone else has added) those who can't teach, teach teachers.

to maintain an abstract approach often obscure the origins of the problems they are designed to handle. The result has been a widening of the intellectual gap between pure and applied mathematics and, regrettably, a virtual estrangement of pure and applied mathematicians. Those who find the precision of set theoretic formulations fascinating often lose sight of mathematics itself, while those who are repelled by formalisms often dismiss all abstractions as mere axiom-pushing and turn a blind eye to the insights that abstraction may provide. The separation begins with the first course adopting the abstract point of view, usually either "modern algebra" or "real variable theory"... Our students... are given the impression that set theory is the ultimate approach to mathematics and that no true mathematician will even consider any question until it has been completely formalized in set-theoretic terms. It is only much later that the student discovers that mathematical research is largely a process of winnowing theorems from a melange of hunches, vague analogies, and geometrical images . . .'

Indeed one can go further than that: one of the richest sources of mathematical discovery lies in the very imprecision of one's ideas and in ambiguity and confusion over notation. (The mathematician says A, writes B, means C, but it should be D: and D is in fact a splendid idea, previously undreamt of, which emerges from tidying up the mess). If you are not too primly corseted by mathematical conventions and nice distinctions, if you are ready to mix freely in the market-places of other men's ideas and to traffic in the wares of other scientific disciplines, then you are more apt to blunder unawares upon a truth inherent in your own subject. The first faint recognition of a new idea is always the hardest step. Its subsequent development, refinement, and presentation in precise and formal language is normally much easier, demanding little more than professional craftsmanship.

Only the undeviating abstract mathematician, not given to applications, would make an unqualified claim that the pursuit of abstract mathematics strengthens one's ability at applied mathematics and the solution of problems. On the contrary, the practice of modern mathematics, no matter what the level of sophistication or how eminent the practitioner, must to some extent diminish a man's powers to apply mathematical arguments to practical situations. I do not mean that, other things being equal, a knowledge of structure and theory is not an advantage. Other things can never be equal: if you spend time and energy on abstract mathematics you will inevitably be influenced by its essential atmosphere and by the attitudes of mind, necessary to its successful prosecution, and you will have less time for the applications and less opportunity to fashion and hone the tools for handling them. Particularly in pure mathematics, much can be achieved by a skilful choice of one's definitions or one's starting point. The much admired quality of 'elegance' can often be secured in this way. But this carries the built-in danger that the subject may develop along the line of least resistance. The mathematician must always be on his guard that the pursuit of lines of least resistance does not become an addiction. The applied mathematician can usefully take note of what the abstract mathematician is up to and what he achieves; but too much attention is a harmful diversion of resources. Thus it is up to the individual tastes and inclinations of each person to make his own choice and to direct his intellectual energies accordingly. And if society expects 'qualified manpower' from its places of education, it will not get its moneysworth unless it decides and secures the right proportion of artists to artisans in the output.

The material on comets in Appendix III may illustrate some of these remarks. The people involved were all at Oxford or Cambridge at the time; and I fear that Appendix III is one of those regrettable Oxford-Cambridge stories which are the better for not being printed. However, I hope you will forgive that if the story serves to show how, in tackling a practical problem, the different skills of several people with different mathematical inclinations can blend and each contribute; how an understanding of theory may be insufficient by itself, and even by its specialization upon the easier soluble cases may not only blunt mathematical technique but may predispose us to overlook interesting solutions; and conversely how numerical solutions of concrete problems can sometimes enlarge the prospects for theoretical investigations. The growing estrangement between pure and applied mathematicians, referred to by Professor Gleason above, should not obscure the interactions of pure and applied mathematics, so well understood and practised by the great mathematicians of the past.

§5.

G. H. Hardy was never particularly keen on applied mathematics: 'the evidence', he writes (8) (p. 72), 'so far points to the conclusion that, in one subject as another, it is what is commonplace and dull that counts for practical life'. There is much truth in this statement, certainly at the more superficial level; but there is also much that it overlooks at the deeper level, as a quotation from Hilbert will show later. Hardy was a superlative pure mathematician, and his views have been influential. Yet in the fairly near future, when an elementary knowledge of biology becomes as common and widespread as an elementary knowledge of physics, he will probably be best remembered for the Hardy-Weinberg law. This law is so simple and fundamental that it appears in the first few pages of any text on genetics, in much the same way as Boyle's law appears in a physics text.* It is the kind of contribution to science which endures, whereas much of Hardy's most beautiful work is already superseded—for example, his work with Ramanujan on partitions was superseded within only a few years by Rademacher's. It is one of those ironic quirks of the history of science that Hardy would have considered the Hardy-Weinberg law commonplace and dull, if indeed he ever bothered to recall it. What we may wonder, was Hardy's ultimate assessment of his life's work, and how far is it summed up in his *Mathematician's Apology?*

* The American Association for the Advancement of Science have listed (in a recent brochure for their journal *Science*) five of the most 'memorable' papers published therein during the twentieth century. One of the five is Hardy's (*Science* 28 (1908) 49-50) and the brochure says: 'this paper by the great British mathematician was the foundation of the statistical theory of population genetics.' By contrast, a committee of the London Mathematical Society, charged with publishing Hardy's *Collected Works*, have tucked this paper away in Volume VII under the heading of Miscellanea. Euler, in a general apology (1741) for mathematics, takes a rather different view from Hardy: he says '. . . the usefulness of mathematics, commonly allowed to its elementary parts, not only does not stop in higher mathematics but in fact is so much the greater, the further that science is developed.' (Quoted by C. Truesdell *Six lectures on modern natural philosophy.* (1966) Springer-Verlag, New York, p. 85).

There are some sombre reflective passages on Hardy, and his intrinsic egocentric similarity to Dag Hammarskjold, in C. P. Snow's essays *Variety of Men* (9) (in particular pages 41 and 155). Snow recounts how Hardy 'lived his own version of a young man's life'—for pure mathematics is, as Hardy himself said, a young man's game—and how 'when his creative powers had finally left him' Hardy attempted suicide.

> 'That is why *A Mathematician's Apology* is, if read with the textual attention it deserves, a book of such haunting sadness. Yes, it is witty and sharp with intellectual high spirits: yes, the crystalline clarity and candour are still there: yes, it is the testament of a creative artist. But it is also, in an understated stoical fashion a passionate lament for creative powers that used to be and that will never come again. I know nothing like it in the language: partly because most people with the literary gift to express such a lament don't come to feel it: it is very rare for a writer to realize, with the finality of truth, that he is absolutely finished. Seeing him in those years, I couldn't help thinking of the price he was paying for his young man's life'.

And Snow also quotes Hammarskjold's words:

> 'Do not seek death. Death will find you. But seek the road which makes death a fulfilment . . . In the last analysis, it is our conception of death which decides our answers to all the questions that life puts to us. That is why it requires its proper place and time—if need be with right of precedence. Hence, too, the necessity of preparing for it'.

I have not said what I mean by modern mathematics. That is partly deliberate, because I think it consists more in an attitude of mind than in a catalogue of subject matter; and attitudes of mind are, like characters in a novel, better not described too exactly but rather left to the reader's own appreciation. That does not make them any less pertinent. But, if I were compelled to agree to a catalogue of subject matter, I should be very happy to accept Dr. Smithies' article(5) as a good basis. Some of the experts on 'modern' school syllabuses might then claim that their syllabuses were not particularly modern in Dr. Smithies' sense, and that they contained plenty of traditional material. I should not want to dispute that. For example, the chapter on social arithmetic in Book 6 of *Modern Mathematics for Schools*(10) by the Scottish Mathematics Group is not modern in Dr. Smithies' sense, nor even very mathematical in any sophisticated sense at all; and yet it contains some excellent educational material which ought to make children aware of the quantitative and commercial aspects of contemporary society. That is all to the good. It is when the new syllabuses turn towards the structural aspects of mathematics that I feel they are taking on the flavour of modern mathematics, and then I become uneasy. Structure is the skeleton which ossifies a subject for all except a few academics. It codifies, and creates a museum piece for appreciation but not for touching or for use. The Eiffel tower is a notable structure; you can stand at the bottom and admire it, or you can go to its top and scan all Paris; but, if you are uncertain of how to get from the Place de la Concorde to the Place Vendôme, there are better ways of finding the quickest route than ascending the Eiffel tower to spy out the land. Or again, a knowledge of the anatomy of the human skeleton will take you only a little way towards understanding man's behaviour, much less his motivations. Pure mathematics must remain a source of aesthetic joy, no more, no less. To that extent, modern mathematics is modern art; whereas applied mathematics is a modern language. The latter is implicit in the following extracts from a précis(11) of a lecture by Sir Cyril Hinshelwood, at that time President of the Royal Society, on the mathematical education of scientists:

> 'Sir Cyril said: "Mathematics, in my opinion, should be at the basis of all science; and mathematics can be taught. It can be taught well or it can be taught badly. What kind of mathematics are going to be most useful to the scientist?" That might seem a difficult question because there were many various kinds of mathematics which found application in different parts of science. You never knew when some abstract branch of mathematics was going to become important. Yet, speaking from the "user" end, one or two things could be said. The key to it all lay in a remark of Willard Gibbs: "Mathematics is a language". And Sir Cyril stated his major thesis as this: "that scientists need to be taught mathematics as far as possible as a language they can actually speak."
>
> We knew that abstract thought without articulate speech was very difficult, if not impossible. We knew the fate that most people suffer at school with the study of Latin: they learn a great deal of Latin grammar, they struggle with proses and so on; and yet not one in a thousand could speak the language at the end of this process, or even write it, for his own purposes. How many children learnt mathematics, too, very much in the manner of the grammar of a language, and were puzzled and repelled by a great many rules and conventions. Yet, when one first came across the use of mathematics to express a real thought and to use the conventions of the grammar to arrive at a result, the whole thing came then as a great revelation.
>
> "There are a great many physical applications of almost all elementary mathematical themes, and these should be brought in at a very early stage of the training. It is of great importance to the scientist to be able to learn the art of formulating problems in mathematical terms, which is of course a quite difficult job. You have to think very accurately and carefully about a problem before you can do it. You have to have had practice in the speaking of the language of mathematics. That is *absolutely vital* to all scientists". It does not matter being expert in differential equations. You can always go to the expert for help in solving an equation. You can state the problem to him straight away in mathematical terms, and his expert knowledge can be brought to bear. But you cannot expect the mathematics to do the translation into mathematics for you. The man of science must be able to think in mathematical terms and speak the mathematical language, though he may not have a vast amount of mathematical technique.
>
> "Casting my mind back, it occurs to me that the parts of elementary mathematics where you can most easily get practice in this art and which, I should have thought, give one the best training for using mathematics in science, are dynamics and statics". There you have a kind of idealised physics. You have the translation of the physical concept into the mathe-

matical symbol, and practice in the use of this language, and you have opportunity for exercise of the quantitative sense. The early development of the quantitative sense is extremely important. Unless people have it developed early they are apt never to acquire it at all. You may find absolutely blank incomprehension among otherwise intelligent people about quantitative arguments. The important thing was to gain early familiarity with the mathematical language: to learn, as it were, French as a child and prattle about childish things in French rather than struggle with it at a mature age . . .

"My thesis is not that there should be an enormous range of mathematics taught, but an early and rather intensive cultivation of the power of thinking about real things and the application of mathematical symbolism to physical ideas". The powers of mathematical expression should be cultivated so that it becomes second nature. Technically advanced mathematics can await time and opportunity'.

§6.

It is often said that educational research has demonstrated the superiority of modern mathematics in the schools. Such claims deserve scrutiny. Let us look at the largest, and some would say the most respectable, of these investigations, namely the recent study of Mathematical Achievement carried out under the International Project for the Evaluation of Educational Achievement(12). The United States Office of Education gave a grant of approximately $250,000 for the international costs of the investigation, covering 12 countries. The numbers of participants in England and Scotland, and the totals for all 12 countries were:

	Students	Teachers	Head-teachers
England	12,740	3,155	684
Scotland	17,472	913	337
All 12 countries ...	132,775	13,364	5,348

We were represented on the Council, directing the project, by the National Foundation for Educational Research in England and Wales and by the Scottish Council for Research in Education. In the summary of conclusions to the report of this project we find (12) (Vol. 2, p. 301):

'Certain test items could be considered to be crucial to the "New Mathematics". If, according to teachers' ratings, most students had an opportunity to deal with items of this category, this was taken as an indication of familiarity with the content of "New Mathematics". At all levels, the students having had "New Mathematics" had a significantly higher mean score on the total tests, even though the results at the pre-university level were not entirely conclusive'.

That looks like strong favourable evidence. Let us pursue it in the body of the report (12), (Vol. 2, pp. 190-194):

'By some mischance the data processing of the material collected in the investigation indicated that no single student in any country had had any course in "New Mathematics". Of course, this is not consistent with the facts . . . This mishap in the data collection was very unfortunate. The hypothesis dealing with the "New Mathematics" courses is interesting and important, and particularly so to mathematics teachers. Thus, it seemed desirable to estimate in some way the population of students who had studied "New Mathematics" . . . The following procedure was used to estimate the desired population of students in an indirect way. From the set of items in each group of tests three were chosen which were both characteristic of the "New Mathematics" and very simple'.

There followed a list of nine test questions (three for each of the three groups). For reasons of space I shall only quote two of these nine; but the interested reader should consult the original report for the other seven questions, as well as those questions in the remainder of the test which were not thought to be so indicative of the 'New Mathematics'. One of the three questions for the group of 13-year-old students was:

'A-23. Which of the following equals $7 \times (3+9)$?
A. $(7\times3)+(7\times9)$ D. 7×27
B. $(7\times9)+(3\times9)$ E. $21+9$'.
C. $(7\times3)+(3\times9)$

and one of the three questions for the group of students in their final secondary year without mathematics as a major subject was

'6-6. Four persons whose names begin with different letters are placed in a row, side by side. What is the probability that they will be placed in alphabetical order from left to right? A.1/120; B.1/24; C.1/12; D.1/6; E.1/4?'

The report continues:

'Concerning these items, it was assumed first that they are so simple that every student who had "New Mathematics" would have encountered them, regardless of the country in which the course was given; second, that very few of those students who have had only "traditional" courses in mathematics will have met these items, since they represent aspects of mathematics which the "New Mathematics" includes and which have been lacking in the traditional courses . . . If the teacher, with regard to all three items which for a particular population were considered to be basic and typical . . . gave the rating A, signifying that most of his students had encountered such problems, then these students were defined as being in the desired population, that is, they were assigned to the population of students who had taken "New Mathematics" courses. It is possible, of course, that the population defined in this way was smaller than the actual population . . . Some teachers, for instance, made no ratings . . . However, in order to get information, this definition was chosen . . . For [the 13-year-old students] the all countries mean of the students who have had "New Mathematics" exceeds the mean of those who have not by a statistically significant amount. Almost all the differences for individual countries are significant. [They are significant for England and Scotland]. A possible explanation is that the "New Mathematics", with its emphasis on the fundamental structural features of elementary mathematics, gives the student a more solid knowledge which he can then use in different kinds of traditional mathematics. For [the group of students in their final secondary year] the data were sparse and the results were inconclusive . . . However, the useable data support Hypothesis 26 [to wit, Hurrah for the New Mathematics] . . . It should, however, be recognized that if only the more able students have had experience with

"New Mathematics", this fact alone could account for their superiority'.

But there is more to it than that: the abler the students in a class are, the more likely are they to have met any given piece of mathematical material, whether or not it typifies 'New Mathematics'. Thus the definition tends to include able traditionalists amongst those classified as 'New Mathematicians' and to exclude the more incompetent followers of new curricula. When we also note that 'sparse' and 'inconclusive' in the body of the report have been elevated to 'not entirely conclusive' in the summary, we do well to listen for the sound of axes being ground.

To the best of my knowledge, there has not yet been anywhere in the world an adequate and reliable piece of operational research into the consequences of teaching modern mathematics. At any rate, as far as Britain goes, we have not yet been teaching new syllabuses for long enough to make such an enquiry possible. Work on writing the new texts began here in about 1961 and it was not until 1967 that the first few students with a school education under the new regime reached the stage of applying for admission to the universities. For the benefit of these few students, we included some additional questions on modern topics in the 1967 Admissions Examination for Oxford. In their comments on this examination, a committee of the Joint Four (i.e. the Association of Head Masters, Head Mistresses, Assistant Masters and Assistant Mistresses) said: 'The questions on "modern" mathematics were easier and therefore presumably did not provide such a searching test of ability'. The examiners replied that they did not regard these questions as easier. Indeed, what emerged from informal discussion amongst the examiners was a general feeling that the answers to the modern questions were less good than to the traditional questions, understanding of basic concepts being somewhat indifferent—for example, some candidates were unable to distinguish the concept of a group from the concept of a set. However, it would be quite wrong to form any firm conclusions on this evidence; for the number of candidates with modern backgrounds was fairly small, and also some of the candidates with traditional backgrounds attempted some of the modern questions. I have sought opinion from one or two of the people prominent in the School Mathematics Project on when it might first be reasonable to undertake a proper statistical analysis of university admissions performance of modern versus traditional candidates. The opinion seems to be that the flow of modern candidates from the schools will be insufficient to allow of such an analysis before 1971. We shall then need to see how these candidates progress during their undergraduate days. At this rate then, we cannot hope to have before about 1975 any sound assessment of the value of modern school syllabuses for those going on to tertiary education.

I am very much in favour of experimenting with syllabuses; for without experiment and innovation, progress is impossible. To that extent, the introduction of modern syllabuses into a number of schools of various kinds is admirable. But until we have clear-cut evidence that modern syllabuses are better than the traditional ones, it seems to me that it would be hasty to abandon the latter everywhere and let the former sweep the country. Can we not go carefully?

§7.

I must leave it to schoolteachers to say whether or not emphasis on abstract mathematics enfeebles mathematical skills at the school level. I am quite sure that it does so at the university level. There it does so because, in stressing generalities, there is less insistence on the solution of particular problems. Amongst the data collected in a survey(13) four years ago of mathematical teaching at Oxford, we obtained undergraduate opinion on tutorials and lectures. Their strictures upon the latter were so severe that we completely recast all the first-year lectures, and arranged for a system of classes as a partial replacement of tutorials. The proposal for these classes, for which weekly examples and problems were set, aroused much controversy, with opinion about equally divided for and against. Half the colleges at Oxford decided to make use of the classes, while the other half continued with the traditional pattern of tutorials. This disagreement was very fortunate because it enabled us to compare at the end of the year the examination results of those who had done the problems with those from the control group who had not, the same examination being set to both groups. In applied mathematics we were (at that time) catering for physicists and engineers as well as mathematicians, and so the problems set in the applied classes were all rather easy. In pure mathematics, on the other hand, there was no such restriction and we could pitch the academic level higher. I happened to be responsible for setting the problems for the analysis section of the pure mathematics, and I decided to set some really tough questions. The consequences were most interesting. In the first place, some of my colleagues, who were conducting the classes, were unable to do some of these problems. Of course, there is no damn merit in that: the setter of a problem is automatically at a great advantage, for having constructed it he knows the trick for solving it. The point at issue is that from time to time I received requests for help, and was therefore in a position to observe the pattern of which kind of mathematician seemed to encounter most trouble. Almost invariably it was those mathematicians who were more deeply involved in abstract mathematics. On the other hand, the sample of dons was small; so the evidence is not weighty. However, what about the undergraduates? Quite a lot of them were all at sea with the harder problems; but not by any means all of them. One of my colleagues remarked to me: 'I can't do your problems at all; but some of the undergraduates can, and I am amazed at the ingenuity they show'. Two things need saying here. First, undergraduates have fewer commitments than dons, and can therefore devote more time to wrestling with a hard problem. Second, mathematics is a young man's game; and, if we believe in progress from one generation to the next, we must all expect our better pupils to be cleverer than ourselves, and accordingly we should give them now and then some tough meat to chew at. And what was the outcome when the examination results came to be analysed? In applied mathematics, where the problems were easy, there was no significant difference between the control group and those attending classes. But in pure mathematics, with the harder problems, those in the classes did significantly better; and their superiority was most marked in analysis where the problems had been the hardest of all. What is more, it

was not merely the best pupils who had benefited: the improvement had occurred fairly uniformly all the way up the scale of ability. The following table gives for the analysis examination paper the percentages in the two groups scoring less than n marks:

Value of n	10	20	30	40	50	60	70	80	90	100
Classes	0	7	15	19	33	51	72	81	92	93
Control	7	22	30	46	67	76	85	95	98	99

(The highest mark obtained in this paper by a class member was 158, against a highest mark of 120 from the control group. There were 72 undergraduates in the classes, against 81 in the control group. The control group had done slightly better than the class group in the admissions examination the previous year, although the difference was not statistically significant; and the analysis examination paper was set and marked by a don who was opposed to the idea of the classes and had not been teaching them). I do not think there is anything extraordinary in these results: they merely show that undergraduates do better when a challenge encourages them to work hard.

I have some reservations about the classes; for, in a sense, they constitute a technical trick for encouraging hard work. An undergraduate education should wean pupils from the necessarily organized work of their schooldays to a stage where they can work on their own. Thus, although classes may be tolerable in the first year of the undergraduate course, I believe that they would be out of place in the second and third years. Particularly in the second year there is a good deal to be said for a little judicious neglect of his pupils by a tutor, thus forcing them if necessary to stand upon their own feet.

I should like to see challenging problems having greater currency amongst schools and universities; and with that in mind I have set some problems in Appendix IV. Some of them are drawn from amongst the problems used in the first-year undergraduate course at Oxford, mentioned above. But I have also included some simpler problems intended for schoolchildren. By 'simpler' I mean that they require less mathematical knowledge, and not that they are necessarily easier. For example, Problem 1 (an elaboration of a problem set to 11-year-olds in Russia) is intended for children at 'O' level; but I do not consider it at all easy. There is not much applied mathematics in the problems in Appendix IV; but they lean towards hard rather than soft mathematics.

I hope that these problems in Appendix IV may be regarded as the first in a series of problems and solutions to appear from time to time in this *Bulletin*; and that readers will be encouraged to send in solutions as well as fresh problems of their own devising. I have tried to cater a little for the proponents of modern mathematics in schools; and Problem 12 is especially intended for them. One of the problems in Appendix IV (I shall not reveal here which one), though based upon a recently published American research paper, can be debunked (like Question B27 in Appendix II); and it was indeed so debunked by at least one of my colleagues and by quite a few first-year undergraduates when (blissfully unaware of these debunking possibilities) I posed it for classwork at Oxford. Maybe some of the other problems in Appendix IV can be debunked as well: let the reader see what he can manage!

§8.

People who attend lectures on vector spaces with an arbitrary finite (or even an infinite) number of dimensions may easily gain the impression that problems in several dimensions are really no harder than in one dimension. This is quite wrong. Topologists tell me that, whereas they know quite a lot about two-dimensional Euclidean space, their ignorance about three or more dimensions appals them. In my own subject (statistics and probability) we are even worse off: we know something about one-dimensional random processes but virtually nothing about processes in two or more dimensions. Lest this statement be misunderstood, I should explain that with, say, a Pólya walk in the plane the plane is only a carrier space for the walk: the space, in which the random process is actually unfolding from step to succeeding step, can be considered as time, which *is* one-dimensional. Two-dimensional 'time' is an altogether more perplexing animal, which, moreover, lurks in some practical problems. For instance, it effectively lies (although this is not superficially apparent) at the root of the difficulty in Problem 16, which is the mathematical formulation of a famous problem in solid-state chemistry, known as the dimer problem. The first part of Problem 16 (existence of the limit) is not too hard and should be within the competence of a good first-year undergraduate; the second part ($e^{G/\pi}$) is very difficult indeed, and a first-rate research student who can solve it may be justifiably well-pleased with himself; and the third part (three dimensions) is a long-standing and celebrated unsolved problem, open to all-comers with no holds barred. The difficulties that increasing dimensionality can create, also appear in a comparison of the two parts of Problem 3, as well as in the three-dimensional analogue (which you may care to tackle) of Problem 8, where the passage has three straight sections each at right-angles to the other two (bent corridor plus lift-shaft, if you like).

The greatest of all mathematicians, Newton, spoke of his own work as that of a man playing with a few pebbles on a beach while the great ocean of truth lay undiscovered before him. I do not know how we should speak of our own age in which mathematicians are so much more numerous and research publications so prolific; but I am tempted to think of a host of sandflies largely congregated into a few small patches of the beach where they mill together in tiny hordes over isolated heaps of sea-wrack—and the ocean, of course, still surges beside them. There is, for instance, an extraordinary concentration of effort upon linear mathematics. Here is Professor Dieudonné(14) on

'. . . the present programme of the first years in the university. There the main topics are: (*a*) Linear algebra in its general form (vector spaces of arbitrary dimension, general theory of matrices and determinants). (*b*) Quadratic forms and finite dimensional Euclidean spaces. (*c*) Derivatives and integrals of functions of several real variables, with their various applications. Differential and partial differential equations. Elementary differential geometry. (*d*) Elementary theory of metric spaces, Banach spaces, Hilbert space and other functional spaces. Elementary functional analysis'.

Many people will regard this as a rather lopsided syllabus, especially if (as Professor Dieudonné suggests elsewhere) it is meant to cater too for engineers. What

is included is for the most part really rather undemanding. But what strikes one most forcibly about it is that, like the examiners mentioned in Appendix II, it lacks discernment between what is and what is not mathematically difficult, and between the well-known and the unexplored. It would be tedious to analyse this syllabus at length; so let us look instead at just a single item in it. We may really wonder if Professor Dieudonné has much conception of the massive difficulties presented by functions of several real variables once one goes beyond the relative trivialities of the linear case. By way of contrast, consider Dr. Thacher's introductory remarks(15) (pp. 679-681) at the conference on *Numerical properties of functions of more than one independent variable*, organized by the New York Academy of Sciences:

'. . . In undergraduate mathematics courses, it is frequently implied that the multivariable case is a rather trivial extension of the single variable problem that is being studied in considerable detail. This is not the case . . . A further difficulty is that much of the existing applicable mathematics is framed in highly abstract terms and thus is not immediately available to those . . . who . . . have entered the field from physical sciences or engineering . . . It may be worthwhile to examine the program [of the conference] not from the standpoint of the subjects covered, but rather from the standpoint of those omitted. I do not refer to the fact that linear algebra and partial differential equations have been so thoroughly discussed that an adequate treatment would require more space than this monograph affords, but rather to the authors who are not represented because they could not be found. Among these important people are: (1) The expert on orthogonal polynomials in more than one independent variable who can . . . tell us something . . . about minimax polynomials analogous to those of Chebyshëv. (2) The mathematician who can give us workable criteria for the existence, and hopefully for the localization, of roots of systems of non-linear algebraic and transcendental equations in several variables. (3) The expert . . . in polynomial approximations to multivariate functions . . . (4) The brother of this expert . . . who knows all about rational approximations in several independent variables. In view of the general lack of understanding of rational approximations in a single variable, it may be some time before this man gets his Ph.D! He will certainly be entitled to one if he solves the problem. (5) The man who really has some better method of tabulating functions of several variables . . . In addition to the men listed above, who may never do any calculating themselves, we need some practical computing men who would talk about such down-to-earth problems as the best algorithms for evaluating multivariable polynomials; the most commonly needed quadrature, interpolation, and differentiation formulas; and the best ways of using tables of functions of several variables. These are areas in which we have as yet insufficient experience to know what really will be useful. This list of problems in the field of multivariate analysis is admittedly incomplete. Many other problems of varying degrees of difficulty will occur to the reader . . . since one of the attractions of this field is the number of opportunities it still offers for significant innovations'.

The difference here is quite simply that Professor Dieudonné is a university mathematician, while Dr. Thacher works at the Argonne National Laboratory. Physicists and engineers want answers to problems and are not content with the superficial generalities that the university mathematician is rather too apt to esteem. It is a very chastening experience for a university mathematician to have to work with some first-rate theoretical physicists, who are naturally very good mathematicians in their own right. I know that, because for a few years I was once the tame mathematician in the Theoretical Physics Division at Harwell.

It may help to put these issues in perspective if we momentarily look aside at a couple of non-mathematical examples of human behaviour.

I learnt from a social worker in England the case history of a marriage which had run on to the rocks. The source of the trouble was the spotted dog. Spotted dog is a sort of pudding, made from dough with currants embedded. You wrap it in a cloth and boil it. It emerges in the shape of a right circular cylinder about three inches in diameter and eight inches long, pasty white in hue and covered with black blotches. If you have the right kind of imagination, it somewhat resembles a small soggy decapitated Dalmatian dog. Perhaps I have failed to make the dish sound appetising. I do not know whether the bridegroom liked spotted dog and had married his bride on that account, or whether he had other motives. Be that as it may, he discovered after his marriage that his wife could cook nothing except spotted dog. 'Imagine it', said the social worker, 'when my own husband comes home from work, he goes into the kitchen and sniffs around and lifts the lids, his curiosity quickened and his appetite aroused. Not so, this other unfortunate husband: he *knew* what was for his dinner, not to mention cold spotted dog for breakfast!' This, you will see, was an educational problem. All the social worker had to do was teach the young bride some other recipes, and the course of love once more ran straight. I tell the story because it affords a true, albeit severe, example of the dangers of overspecialization.

The second example you may experience for yourself if you fly the Atlantic by an American airline. You can then watch 'in-flight movies': you can watch them, but you cannot hear the sound-track unless the air-hostess persuades you, for a 'modest sum', to acquire a pair of ear-plugs (like those of a stethoscope) attached to a wire that fits into a terminal on your chair. If you decline her offer you can study instead the reactions of your fellow passengers, almost all of whom will be dutifully plugged in. Emotions of mirth, surprise, and so on pass across their faces in strict synchronism with the unfolding Hollywood parade. Between these simultaneous and corresponding flashes of expression, there are gentler interludes when everyone is transfixed by a ruminant watchfulness—much enhanced in the case of those chewing gum. It is a strange and eerie mid-twentieth century experience, not at all like the concerted sympathetic reactions of a theatre audience; for each participant sits in silence isolated from his neighbour at the end of his own electronic umbilical cord—*homo acquiescens* bowling along at 500 knots in a busload six miles above the endless white horses and the spume.*

I do not think it is really an exaggeration to call Professor Dieudonné's specimen syllabus a diet of

spotted dog, for it is a gross overspecialization on but a single facet of mathematics. I think also that a section of the mathematical community in universities is just *plugged in* to the sounds of the Bourbaki bandwagon. Just as many an airline passenger is subtly induced to go on listening to the simulacra of reality because he has paid for his ear-plugs, so some mathematicians, having expended a modest amount of intellectual effort on Bourbaki or the like, are loth to ditch these pasteboard orthodoxies. And the more often they rehearse them, the more conditioned their reflexes against applied mathematics. If you check up on the mathematical qualifications of those who have been active in preaching modern mathematics to schoolteachers—for example, those who ran Academic Year Institutes in the United States—by looking in the index of *Mathematical Reviews* to see what they have published, you will discover that (with only a few interesting exceptions) they have published little, or more usually nothing. Those who can't, expound: and they expound uncritically and acquiescently the orthodoxies which they have gone to some trouble to learn. There are many wild theories about that mythical character, Bourbaki: that he exists because nobody would wish to peddle such stuff without the cloak of anonymity; or that the whole Bourbakiste movement is a Gaullist machination to stifle American technology. In that, however, the French may have quite unintentionally succeeded; for the present practice of modern mathematics in the American schoolroom has probably blunted the mathematical edge of a whole generation of children to an extent that we do not yet quite realize.

§9.

I find myself driven to admit that what seems most to characterize the abstract university mathematician is his intellectual *innocence*. I believe there are in Britain two main causes, which are effectively two sides of the one coin.

First, there is the postgraduate system which trains him. Although there has recently been a limited cutback, the state still provides postgraduate grants on a generous scale. A first class honours degree is not far from being an automatic qualification for a postgraduate grant, and quite a few people qualify with only a good second class degree. The number of postgraduate students is therefore considerably greater than the number who possess the originality of mind needed for genuine research. That would not matter if, quantitatively at least, the main intention of postgraduate studies was instruction rather than research. While many postgraduate courses do certainly begin with instruction, leading to a master's degree, and while a good proportion of students rightly stop after that stage, it is still the case that too many of them continue towards a doctorate.† Little wonder then that doctoral theses are too often pedestrian. Moreover, grants have a time scale. A supervisor knows that his postgraduate student's grant will run out at the end of (say) three years; and he must therefore select a safe topic for the thesis, safe in the sense that he can be pretty sure that three years' conscientious work will produce something that can be written up. Even with brilliant pupils, he is taking a definite risk if he chooses a topic which, as genuine research usually entails, is open-ended without any sure expectation of publishable results. And for a bright student, whom he does let loose on the more exciting unknown, he will be wise to retain some safe subject in reserve to save the student from the catastrophe of having nothing to show on paper when his grant expires. In mathematics, 'translation' is a reliable and popular method of achieving safety: in this, one takes some well-known old-fashioned mathematical theme and translates it into the fashionable jargon of the moment (thus 'modernizing' it). For preference the original theme and the new jargon frame should be selected to permit generalization, for this adds respectability and an aura of having produced something new—that is to say not ostensibly in the prototype. For these purposes, it does not matter much if the generalization is uninteresting or unimportant: it suffices if no one else has bothered with it before. Abstract spaces, measure theory, categories, matroids, functional analysis, and so on all offer happy hunting grounds for the superficial generalization. Thus, we read(16):

> 'At its best functional analysis unifies many seemingly diverse situations in a wonderful way and is a genuine principle of research. At its worst it is a scintillating wrapper that provides attractive packages, and camouflages with glamorous language the fact that their content may be small or may have been originally obtained in the drab workshops of hard analysis'.

In research there is too much re-search, too little search. Let us not imagine for a moment that the humdrum postgraduate cannot produce the most appallingly ordinary doctoral dissertation in applied mathematics: that certainly happens too; but, for one reason or another, it is commoner in pure mathematics. One possible reason, which may be worth suggesting, is the fact that the population of postgraduate students contains a higher than average proportion of brink shiverers. The clever undergraduate, who secures a first or a good second in mathematics, has quite frequently achieved this result by focusing most of his energies on mathematics; and suddenly he finds himself faced with the alternative of striking out into the world at large with all its alarming strangeness, or of continuing with a research grant along familiar lines. He shivers on the brink, and chooses the latter; and, when the same choice presents itself again on getting his doctorate a few years later, he may easily shiver himself into a university appointment. And now, there is no more guaranteed recipe for the production of tame theses than to have as your supervisor an authoritarian professor, who was a brink-shiverer in his younger days and was never himself capable of anything more than a cautious thesis com-

* I saw some refinements four weeks ago on the polar flight from Los Angeles to London. Shortly after the film had ended, maybe over Baffin Island, an engine malfunctioned and we diverted to New York to get a fresh aeroplane. Now what goes on inside the cabin is linked inexorably, it seems, to the take-off drill: the plane takes off, passengers may unfasten their seat-belts, whereupon they are dosed first with cocktails, then a meal, and finally the film. We went through all this once more on taking-off again from New York. But a fresh aeroplane doesn't imply a fresh film: same film as four hours ago, same passengers plugged in, same simultaneous expressions (conditioned reflexes *this* time). Await the twitch upon the thread!

† In the report of H.M. Civil Service Commissioners for 1967 we find: '. . . The number of candidates with higher degrees continues to increase; regrettably, however, many do not appear to have derived sufficient benefit from their extended stay at university and a Ph.D. is not necessarily a reliable pointer to suitability for the Scientific Officer class. There has also been a considerable increase in the number of candidates with Third Class Honours and Pass degrees; a significant proportion of these would have been better fitted for employment if their education and training had been more concerned with practical application.'

posed of unexceptionable generalizations. That may sound savage and unworthy; but unfortunately it happens occasionally, and who stands more in need of protection—the innocent student or the innocent professor? Any prospective research student, who cannot take the trouble to go into a library and ferret out* what his prospective supervisor has previously published in the way of research, shows himself unfit for research (which involves library searches) and deserves his fate. It is a pity that the Ph.D. has become a sort of union ticket for university appointments. The best reason for doing research is curiosity, a desire to know the answer, and to promote knowledge. Things were much healthier when a pupil could merely hang around a distinguished researcher, getting the feel of the thing, and able to go away without loss of face if he found things not to his taste or capacity. A man can perfectly well start on research after taking up his first academic appointments; and, if he is not tied to a specific subject for a thesis, he can indulge his curiosity more freely and profitably. All that is needed is sufficiently many temporary junior appointments. Equally the rat-race to promotion by weight of publication rather than content is an unhappy modern development. Dr. A. K. Austin's satire(17) is uncomfortably close to the truth:

> 'The advent of Modern Mathematics in the educational limelight has produced an interest in the work of the professional mathematician and in the question, "How can one do research in mathematics?" The following passage formed the introduction to a recent research paper and indicates to some extent the line taken by a number of mathematicians . . . A. C. Jones in his paper "A Note on the Theory of Boffles" . . . asked if every Biffle was reducible. C. D. Brown . . . answered in part this question by defining a Wuffle to be a reducible Biffle and was then able to show that all Wuffles were reducible . . . T. Brown in "A collection of 250 papers on Woffle Theory dedicated to R. S. Green on his 23rd Birthday" defined a Piffle to be an infinite multivariable subpolynormal Woffle which does not satisfy the lower regular Q-property. He stated, but was unable to prove, that there were at least a finite number of Piffles . . .'

You may not believe in boffles and piffles; but some very quaint titles do crop up. On the same day as I write this (so it is not an unusual event) I have received a paper in which there is a (genuine and respectable) reference to 'Generalized rabbits for generalized Fibonacci numbers', *Fibonacci Quarterly* (in the press).

All this, then, is 'modern art'; and no society worth its salt would wish to suppress art. The only question is one of extent: how large a subsidy should the taxpayer provide? Mixed up with this modern art, there is much of the 'modern languages' side of mathematics, on which the wealth and survival of the country indirectly hang. Prune the art too much, and you may stunt the language side. Striking the right balance is a delicate and technical business, likely to be bungled if left to politicians and publicists and administrators. The only sane process is for the mathematical profession itself to show due responsibility in dispensing public subsidies. Any section of society, which turns in upon itself and indulges too fully in art and mannerisms, in self-entertainment and petty assessments over advancement and status, contains the seeds of its own destruction, and superb artistic work achieved or unborn may perish along with it. On the court of Louis XIV and Versailles, Bossuet himself (18) (p. 95) wrote: '*Cette ville de riches aurait beaucoup d'éclat et de pompe mais elle serait sans force et sans fondement assuré . . . et cette ville pompeuse, sans avoir besoin d'autres ennemis tomberait enfin par elle-même, ruinée par son opulence*'. That analogy is overstated, yet better not ignored.

§10.

The second reason for the innocence of the abstract university mathematician is his reluctance to tackle problems, especially practical problems. There could be food for thought in a historical critique (if someone would write it) on the mathematical usage of words like 'regular' and 'normal' and their antonyms. 'Regular' may mean 'systematic and symmetrical' (regular polygon), or it may mean 'conformist and well-behaved' (regular function). 'Normal' may mean 'perpendicular' (normal to a curve), or it may mean 'conventional and amenable' (normal topological space). Have there been historical trends in the meanings of such words? Who first christened the Gaussian distribution 'normal', and when and why? When he defines a normal space in his book *General Topology*(19) (p. 112), Professor J. L. Kelley comments:

> 'This nomenclature is an excellent example of the time-honored custom of referring to a problem we cannot handle as abnormal, irregular, improper, degenerate, inadmissible, and otherwise undesirable. A brief discussion of the abnormalities of the class of normal spaces occurs in the problems at the end of the chapter'.

But is this prevalent custom really 'time-honored', or is the shirking of difficult problems only a passing contemporary sickness? Let us look at what four of the great mathematicians of recent years have said about difficult problems and applications. First Hilbert(20):

> 'As long as a branch of science offers an abundance of problems, so long is it alive; a lack of problems foreshadows extinction or the cessation of independent development . . . It is by the solution of problems that the investigator tests the temper of his steel . . . The mathematicians of past centuries were accustomed to devote themselves to the solution of difficult particular problems with passionate zeal. They knew the value of difficult problems . . . Surely the first and oldest problems in every branch of mathematics spring from experience and are suggested by the world of external phenomena . . . But in the further development of a branch of mathematics, the human mind, encouraged by the success of its solutions . . . evolves from itself . . . new and fruitful problems, and appears then itself as the real questioner . . . In the meantime, while the creative power of pure reason is at work, the outer world again comes into play, forces upon us new

* Since this article may be read by prospective research students who do not know quite how to proceed, some suggestions may not be amiss. (i) See what books your prospective supervisor has published; but remember that book-writing is not a very good indicator of research, since it may (quite properly) amount to potting other people's ideas. (ii) List his journal publications by consulting the author indexes for *Mathematical Reviews* for the last 10 years or so. If he is an applied mathematician, you will also want to consult *Physics Abstracts*, *Chemical Abstracts*, etc. as appropriate. (iii) *Read the papers* listed as a result of your activities in (ii), and follow up the references in these papers too to see how your prospective supervisor compares with other people in the same field. (iv) Cross-examine his present or past research students. (v) Finally get a personal interview with him, and remember that you are interviewing him as well as he you. A few weeks' work spent on vetting a prospective supervisor is well worth the trouble: after all, you are liable to be stuck with him for the next three or four years!

questions from actual experience, opens up new branches of mathematics . . . And it seems to me that the numerous and surprising analogies and that apparently prearranged harmony which the mathematician so often perceives in the questions . . . have their origin in this ever-recurring interplay between thought and experience'.

Next von Neumann[21]:

'As a mathematical discipline travels far from its empirical source, or still more, if it is a second or third generation only indirectly inspired by ideas coming from "reality", it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely *l'art pour l'art*. This need not be bad, if the field is surrounded by correlated subjects, which still have closer empirical connections, or if the discipline is under the influence of men with an exceptionally well-developed taste. But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganized mass of details and complexities. In other words, at a great distance from its empirical source, or after much "abstract" inbreeding, a mathematical subject is in danger of degeneration. At the inception the style is usually classical; when it shows signs of becoming baroque, then the danger signal is up.'

Thirdly, Hermann Weyl[22]:

'Important though the general concepts and propositions may be with which the modern industrious passion for axiomatizing and generalizing has presented us, in algebra perhaps more than anywhere else, nevertheless I am convinced that the special problems in all their complexity constitute the stock and core of mathematics; and to master their difficulties requires on the whole the harder labor.'

And Pólya[23] (pp. vii and x) writes:

'Solving problems is a practical art, like swimming, or skiing, or playing the piano: you can learn it only by imitation and practice . . . Our knowledge about any subject consists of *information* and *know-how*. If you have genuine *bona fide* experience of mathematical work on any level, elementary or advanced, there will be no doubt in your mind that, in mathematics, know-how is much more important than mere possession of information. Therefore, in the high school, as on any other level, we should impart, along with a certain amount of information, a certain degree of *know-how* to the student. What is know-how in mathematics? The ability to solve problems—not merely routine problems but problems requiring some degree of independence, judgment, originality, creativity. Therefore the first and foremost duty of the high school in teaching mathematics is to emphasize *methodical work in problem solving*. That is my conviction . . .'

At a much humbler level it is also my own conviction, first acquired from my own schooldays. My interest in mathematics was first aroused when I was eleven years old. My teacher at that time, the late Mr. Gerald Meister, taught us dexterity in manipulation, and speed of working. And he would set us problems of all sorts and kinds, in particular Caliban's weekend puzzles which used then to appear in *The New Statesman*. I cannot recall him laying stress on mathematical concepts or structure; our appreciation and understanding of these things only seeped in later as it were by osmosis from the practice we had in doing the mathematics. Ever since, I have been convinced that the important thing is to *do* mathematics, understanding afterwards what one has done. I am also convinced of the great value of problems in examinations, to be tackled within a time limit. That teaches you to go quickly to the essentials of the matter, and to possess and to summon up mathematical techniques, held in incisive readiness. There are many books on puzzles and problem-solving which schoolteachers can profitably use: references[24] [25] and [26] are a suggestion for a start.

I shall end as I began by quoting from the prose works of a poet. For lecturers and teachers there is something fit in the title of John Masefield's fragment of an autobiography *So Long to Learn*, from which I take this[27] (p. 149):

'I asked a writer what good advice he could give to a beginner like myself. He said "Practise all kinds of writing . . . writing much every day, on all sorts of subjects, and making yourself do it against time. That will make you able to marshal your thoughts and state your mind clearly and at once". I was amazed at this reply, and disapproved of it at the time, for he had ever seemed to me to be opposed to any hasty composition, to write with extreme care and much revision, and this care was then an article of faith with us. Later I learned that one of the needs of the young writer was (and is) to get rid of literature, to learn concision, to know the power of the noun and the limitation of the adjective. Much writing, self-imposed, against the clock, will help him in this necessary discipline'.

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Appendix 1 The squirrels of set theory

A good mathematical notation is legible and lends itself to manipulation. In algebra, sums and products are governed by a *binding convention*: thus, in the product xy the two symbols are written close together to suggest tight binding, while in the sum $x+y$ they are written further apart to suggest looser binding. This binding convention lets us write $ab+cd$ without fear of confusion with $a(b+c)d$. The notation $\cup$ and $\cap$ of set theory is a poor one. First, the two symbols are typographically rather alike; so it is hard to read a long formula containing a mixture of them. Second, they lack a binding convention; and therefore formulae become cluttered up with unwelcome brackets to deal with ambiguous things like $A\cap B\cup C\cap D$. The point is unimportant for theorists who rarely meet with extensive manipulations of sets; but it is sometimes important for applied mathematicians, for example in complicated probability problems. Because of the brackets, a formula in set notation can often be twice the length of the same formula in algebraic notation. When I was an undergraduate at Cambridge my supervisor taught me: 'If you want to know whether a formula is right or wrong, take a ruler and measure it: if it is more than 15 centimetres long, it is wrong'. (He was referring to Delauney's theory of the moon in which there are three formulae respectively 137, 173, and 155 quarto pages long(28) (vol. 28, pp. 119-256; vol. 29, pp. 241-413; vol. 29, pp. 415-569)). In my own private work I always use $A+B$ for $A\cup B$ and AB for $A\cap B$, and I invoke the binding rules of algebra. If necessary, I begin by translating from set notation to algebraic notation and end by translating back again for purposes of publication. I daresay many other people do so too*. There is, I believe, a law in New York State that you may not kill troublesome squirrels, though you may catch them and release them alive three miles from the point of capture. Meanwhile your neighbour is releasing his squirrels on your land.

* But Professor Moran, in his splendid new book(29) *publishes* in algebraic notation. Three cheers!

Appendix II Remarks on some recent examination papers set for the School Mathematics Project

Let us look at two particular examination papers selected from those set recently for the School Mathematics Project by the Oxford and Cambridge Schools Examination Board, namely the Ordinary Level Elementary Mathematics I paper for December 1965 and the Advanced Level Mathematics II paper for July 1966. It may be that these two papers, representing early attempts to examine a new syllabus, are not typical of what goes on in the schoolroom or what will be examined in future years—indeed, I hope so; but I had better confine myself to these as typical of the evidence available to me.

The impression that I get, particularly from the Advanced paper, is that the examiners lack discernment of what constitutes a difficult mathematical problem and what does not. The paper does indeed contain some difficult questions; but it also contains some very easy ones, and it is so constructed that a candidate can get full marks by confining himself to the latter. To this extent at least it offers a soft option. The Advanced paper is divided into two sections*, Section A with 20 questions and Section B with 8 questions. The candidate has 3 hours for the whole paper and the rubric reads: 'Candidates must not attempt more than 12 questions from Section A or more than 4 questions from Section B. Candidates are strongly advised not to spend more than half the time on Section A'. From this one might conclude that the candidate might well spend 2 hours, or perhaps a little less, on Section B. Yet, by choosing the four easiest questions, I think a good candidate ought to be able to dispose of Section B in only a quarter of an hour†, as follows:

Time to read the eight questions in Section B and to select the four easiest questions		8 minutes
Time to answer question	B22	1 minute
	B24	3 minutes
	B25	2 minutes
	B27	1 minute
	Total	15 minutes.

For example, consider question B27:

'A sequence a_i ($i=1, 2, 3, \ldots$) is defined by $a_1=a_2=2$ $a_n=a_{n-1}+a_{n-2}$ ($n=3, 4, 5, \ldots$). Prove that $a_i=0$ (mod 10) when i is a multiple of 5'.

Admittedly, to do this question one has to understand the jargon involved with mod 10; but, given this knowledge, the solution is very simple:

'Solution; The final digits of $a_1, a_2, \ldots$ are 2, 2, 4, 6, 0; 6, 6, 2, 8, 0; 8, 8, 6, 4, 0; 4, 4, 8, 2, 0; 2, 2, . . . after which the pattern repeats'.

Quite possibly the examiner was looking for some erudite stuff on the congruential properties of Fibonacci sequences; but if so, he should be disappointed, and rightly disappointed because it is out of place to make mountains out of molehills in mathematics. Compare this question with question B28:

'You have positioned yourself on the boundary so as to catch a ball hit by the batsman. Show that, if it were not for the effects of perspective, air resistance and a few other realities, the ball would appear to be rising vertically with a constant speed, all the time. Describe, as carefully as you can, how air resistance would affect this result'.

This is an interesting question: the first part is straightforward, but the second part seems to me to be pretty difficult for A-level. I shall discuss this question more fully at the end of this Appendix.

In Section A of this paper there is a notable contrast between the abstract questions:

'Question A13. Prove that the set operation of union is associative. (A demonstration by Venn diagrams is unacceptable)'.

and the questions which bear on practical things, such as:

'Question A12. Two unequal electrical resistances combine in series to give a resistance R_s and in parallel to give a resistance R_p. Prove that $R_s > R_p$'.

To this, the following solution

'Starting with the resistors in series, first short the first resistor, and then shunt it (unshorted) across the second. Each operation obviously reduces the effective resistance'.

might not (I suspect) receive a great deal of credit from an examiner who is looking for a derivation from the formulae for resistors in series and parallel, though to my mind it is an answer which exhibits the right kind of physico-mathematical understanding of how circuits behave. Also consider

'Question A16. The mean survival period of daisies after being sprayed with a certain make of weed killer is 24 days. If the probability of survival after 27 days is $\frac{1}{4}$, estimate the standard deviation of the survival period'.

* I have subsequently heard that this practice of dividing the Advanced paper into two sections has been abandoned in more recent years.

† I have been told that these times are unrealistic and that a schoolchild could not write out the answers so quickly even if he already knew what to write. I do not feel inclined to argue the point; but for the record I will state that I obtained the times by timing myself at doing the questions including writing out the answers, after which I approximately doubled my own times. For example, my actual time for question B27 was 35 seconds.

Now, is the candidate expected to apply some cookbook method which assumes that survival times are normally distributed; or is he to try to make his mathematical model realistic? If the latter, then he will have to remember that the distribution is a mixture of two components—the lifetimes of daisies which escape all effects of the spray and of those which do not—and neither of these two distributions are likely to be normal; and he is up against some pretty awkward mathematics. The question hardly suggests that the examiner has extensive experience of constructing mathematical models.

In the S.M.P. Ordinary Level paper we have some multiple choice questions, and the candidate has to encircle the letter or letters corresponding to any correct answer. Question 16 runs as follows:

'"Passengers are allowed 40lb. of luggage free of charge; any amount in excess of 40lb. is charged at 3d. per lb." If W lb. is the weight of the luggage (W is an integer) and C shillings is the cost, the regulation quoted above is equivalent to

(a) $C=3(W+40)$; (b) $C=\frac{1}{4}(W-40)$;
(c) $C=40+3W$; (d) $C=\frac{1}{4}W-40$.'

Since all four choices are false, how does the examiner distinguish between a candidate who answers the question correctly and one who does not attempt it at all? The notable point about this question is that there is no need to scrutinize the individual coefficients in these formulae: all the formulae are linear, and therefore obviously false since the situation is non-linear. Here then we have an example of a pundit of modern mathematics, with all its claptrap about linearity and linear vector spaces, quite unable to recognize whether a simple practical situation is linear or not.

Next consider Question 23:

'$\mathscr{E}=\{1{\cdot}6, 5{\cdot}1, -3{\cdot}5, -2{\cdot}4, 0{\cdot}3\}$. (i) Write down the number $x \in \mathscr{E}$, such that $-2<x<0$. (ii) Write down the member of $\mathscr{E}$ which is nearest to 1·6 (other than 1·6 itself). (iii) Find $a, b, c \in \mathscr{E}$, such that $a+b=c$'.

What exactly is the purpose of set-theoretical language in a case like this? Would it not be better to ask:

'From the five numbers 1·6, 5·1, –3·5, –2·4, and –0·3 (i) write down the one between –2 and 0; (ii) write down the one nearest to but not equal to 1·6; (iii) determine which one is the sum of which two others?'

That, at least, would expose the sheer triviality of the question. In Question 26, the examiner seems to be at sea in manipulating inequalities:

'It is given that $\frac{1}{a}=\frac{1}{p}+\frac{1}{q}$ (i) Write the right-hand side as a single fraction in terms of p and q. (ii) If $p=\frac{1}{2}$ and $q=\frac{1}{4}$, find a. (iii) If $a > p$ state, with a reason, whether q is positive or negative'.

The answer to (iii) depends upon whether a and p have like or unlike signs; so it is quite a logical puzzle and a test of brevity for the good candidate to squeeze his answer, together with a reason, into the space provided; namely two dotted lines each one inch long. Finally, hidden amongst the relative triviality of the other questions, there is Question 29, which asks the candidate to inscribe a square in a triangle and tells him to leave all construction lines visible so that his method is clear. This question, especially the bit about making the construction clear from the diagram, is pretty difficult—unless, of course, it happens to be a piece of bookwork in one of the S.M.P. manuals.

Finally I should like to return to the cricketing question, B28, previously mentioned in this Appendix. Suggestions on how to tackle this question are, of course, irrelevant to my criticism of these S.M.P. papers; but the question is, as I have said, mathematically interesting and it has, I understand, caused quite a lot of discussion amongst schoolteachers. Although the question is much too demanding for an A-level examination, or even for a scholarship examination, I believe it could well afford good opportunities for classroom discussion, where more time is available and where the teacher can provide guidance and explanation; and I hope some schoolteachers will care to take it up with their better pupils and may find the following analysis helpful as a lead-in. I make no claim that it is a complete answer to this A-level question, still less that it is what the examiner was looking for. I have, incidentally, discussed this particular question with one of the leading lights in the S.M.P. movement. He felt that a candidate was only expected to draw a rough sketch of the shape of a trajectory affected by air resistance, and to argue qualitatively from that sketch. The trouble about that line of attack is the difficulty of allowing for the varying speed of the ball along its trajectory; and, indeed, when he applied this method to the problem, he arrived at conclusions contradictory to those of mine below. I must leave it to the reader to decide for himself whether or not my conclusions are right, as far as they go.

If the distance between the batsman and the fielder is a, and if the horizontal component of the ball's velocity is u, then the ball will appear to rise with a constant speed $\frac{1}{2}ag/u$ in the absence of air resistance. One might hope that this result would still be approximately true in the presence of air resistance with the qualification that u, and hence $\frac{1}{2}ag/u$, will no longer be constant. To investigate this, take Euclidean co-ordinates in the vertical plane, with the batsman at the origin, the fielder at $(a,0)$, and the ball at (x, y). Let (u, v) be the components of the velocity of the ball; and write $u=u_x$ when we wish to stress that u is a function of x. The apparent height of the ball above the batsman will be $h=h_x=ay/(a-x)$, by similar triangles. Suppose that the resistive force is always tangential to the trajectory (for example, the ball is spherical and not spinning) but is otherwise arbitrary. Using dots to denote differentiation with respect to the time t, we have $\dot{u}=-uf$, $\dot{v}=-vf-g$ where f is some arbitrary function of position, velocity, and so on. Hence

$$\frac{d^2y}{dx^2}=\frac{1}{u}\frac{d}{dt}\left(\frac{v}{u}\right)=\frac{u\dot{v}-v\dot{u}}{u^3}=\frac{-g}{u^2}=\frac{-g}{u^2_x};$$

and the solution of this differential equation is

$$y=A+Bx+\int_0^x\left(\int_0^z\frac{-g}{u^2_w}\,dw\right)dz=A+Bx-g\int_0^x\frac{(x-z)dz}{u^2_z}$$

the last expression resulting from integration by parts. The arbitrary constants of integration, A and B, must satisfy the condition that $y=0$ when $x=0$ and when $x=a$; and this leads to

$$y=\frac{gx}{a}\int_0^a\frac{(a-z)dz}{u^2_z}-g\int_0^x\frac{(x-z)dz}{u^2_z}=\frac{g(a-x)}{a}\int_0^x\frac{zdz}{u^2_z}-g\int_x^a\frac{(z-x)dz}{u^2_z}$$

Consequently the apparent speed of rising is

$$\dot{h}_x=u_x\frac{d}{dx}\left(\frac{ay}{a-x}\right)=-agu_x\frac{d}{dx}\left[\frac{1}{a-x}\int_x^a\frac{(z-x)dz}{u^2_z}\right]$$

$$=\tfrac{1}{2}agu_x\left[\int_0^{a-x}\frac{zdz}{u^2_{a-z}}\Big/\int_0^{a-x}zdz\right]$$

This is $\frac{1}{2}agu_x$ times the weighted mean of $1/u^2_{a-z}$ over $0\leqslant z\leqslant a-x$ with weight z. Since u_x obviously is a non-increasing function of x, we get

$$\tfrac{1}{2}ag/u_x\leqslant\dot{h}_x\leqslant\tfrac{1}{2}agu_x/u^2_a$$

with a suggestion that the lower bound may be quite a reasonable approximation in view of the increasing weight z. Moreover this approximation will improve towards the end of the trajectory where $a-x$ becomes small; and right at the end of the trajectory we get the exact result $\dot{h}_a=\frac{1}{2}ag/u_a$. There is, indeed, a good deal more that can be said about this problem; for example, one may look at special cases with particular resistance laws (thus it is straightforward to find u_x and $\dot{h}_x$ explicitly when the resistance is proportional to the speed of the ball) and examine numerically the adequacy of the approximation $\frac{1}{2}ag/u_x$ and compare it with other possibly better approximations such as $\frac{1}{2}agu_x/u^2_{(a+2x)/3}$. (The latter approximation is the result of replacing $E(1/u^2_{a-z})$ by $1/u^2_{a-E(z)}$ where E is the operation of taking the relevant weighted mean). One of the main virtues of the problem is that it provides plenty of opportunity for awkward manipulations and numerical investigations: these are the essential stuff of practical mathematics, where a fearlessness of manipulation is *far* more valuable than a knowledge of mathematical theory. There are three levels of mathematical activity. The lowest level consists in following somebody else's argument, say in a book or a lecture; and it calls for little more than mathematical knowledge, whether or not the subject matter is elementary or advanced. The second and more demanding level is most frequently met in examinations, and consists in constructing an argument to prove some stated result. The third and most difficult level is to invent and discover the results that have to be proved. Progressing from the first to the third level, that is from the world of learning to the world of doing, calls for increasing powers of manipulation and imagination and relies less and less on established theory.

Appendix III A spot of trouble with integral equations

About 10 years ago, Dr. R. A. Lyttleton propounded a problem on the lifetimes of comets(30). The familiar comets (like Halley's) which have short orbital periods of a few tens of years are very much in the minority amongst the population of all comets. The great majority of comets, of which there are something like one or two million, describe very long narrow elliptic orbits about the sun with periods of the order of 100,000 years. In any given year we can only observe a few of them, namely those which happen to be in the neighbourhood of the sun and planets at the time. At such a time a comet also comes under the gravitational influence of

the major planets, Jupiter and Saturn; and this disturbance may sometimes convert the comet's elliptic orbit (which is already almost parabolic) into a hyperbolic orbit, in which case the comet escapes from the solar system. This event can be considered as effectively random because there are so many comets in the population, and their movements are not correlated with those of the planets. The problem is to determine the statistical distribution of lifetimes of comets up to their moments of escape. Dr. Lyttleton consulted Professor Lindley, who formulated a mathematical model for him in terms of probability theory and showed that the answer to this depended upon solving an integral equation of the form

$$P(t,z)=\begin{cases}\displaystyle\int_{-\infty}^{0} g(u-z)du+\int_{0}^{\infty} P(t-u^{-3/2},\ u)g(u-z)du \\ \qquad\qquad \text{if } t\geqslant 0,\ z\geqslant 0 \\ 0 \quad \text{if either } t<0 \text{ or } z<0.\end{cases}$$

Here g is the given function $g(x)=(2\pi)^{-\frac{1}{2}}e^{-x^2/2}$, and P is the unknown function which has to be determined. So Dr. Lyttleton went to Dr. Smithies, as an expert in integral equations and learnt that the available theoretical work on integral equations did not cover situations like this. Next he tried Professor J. E. Littlewood, whose advice was: 'There's some chap at Oxford who deals with these things by non-rigorous methods; but I've forgotten his name'. This apparently being a recognizable description of me, the documents duly turned up on my desk, and after a good deal of tinkering with the problem I did fortunately manage to deal with it[31] by a combination of three different methods, only one of which concerns us here. This method was an asymptotic argument, which effectively linearized the problem and led to the much simpler integral equation

$$c(x)=g(x)+\int_0^\infty c(u)g(x-u)du. \qquad (1)$$

This is closely related to the well-known Wiener-Hopf equation, and I had high hopes that known theoretical techniques would apply to it. But difficulties soon appeared; and accordingly I took this latter equation (1) to Professor Fox for advice on its numerical solution on a computer, for which Mr. K. Wright wrote the program. The outcome of this is described by Professor D. G. Kendall, in his account[32] of his parallel work on this problem.

> 'More general statements would be possible if we had more information about the range of solutions to the Wiener-Hopf equation; the existing studies of this equation (for example, F. Smithies[33]) depend upon hypotheses not always satisfied in our problem. A very thorough study[34,35] by Spitzer of the Wiener-Hopf equation when the kernel is a probability density at first appears very suitable for our purposes, but unfortunately it is only concerned with the monotone solutions to (1), and this is a serious defect from our point of view because of the remarkable discovery by Wright and Hammersley that c ( ) itself need not be monotone . . . The (very surprising) ripple in the values of c ( ) rapidly decays and ultimately the solution settles down to the limiting value 1·4142. As this is a bounded solution there can be no question of its not being the right one. That this is the correct solution . . . can also be shown by calculating numerically [the integral $\int_0^\infty c(x)dx\int_x^\infty g(y)dy=\frac{1}{2}$] using the computed values of $c(x)$. We obtain 0·5000. Any admixture of another solution would have raised this to a value greater than $\frac{1}{2}$. Also it is interesting to notice that the initial and final values agree with the predictions of our theorems 4 and 5. Theorem 4 tells us that when $x\to\infty$ then $c(\cdot)$ is $(C, 1)$– limitable to the limit $\sqrt{2}$ ($=1{\cdot}41421356$). Theorem 5 tells us that when $x\to 0$ then $c(\cdot)$ is $(C, 1)$– limitable to the limit . . . $(2\pi)^{-\frac{1}{2}}\zeta(\frac{3}{2})$ = 1·04218698. Here $\zeta(\cdot)$ denotes Riemann's zeta function. Thus the solution computed by Wright and Hammersley takes on the correct values at the two extremes of the range'.

Dr. D. C. Handscomb was also able to show that a limiting discrete analogue of (1) must be satisfied by a rapidly damped oscillation with a principal period $(2\pi)^{\frac{1}{2}}$, which agreed with the spacing between the maxima of the computed results for c. I have quoted the passage from Kendall at some length partly because it shows that theory provides relatively weak conclusions, in this case $(C, 1)$– limits instead of the values themselves, but mainly because it shows the reservations and caution which a pure mathematician feels when an electronic computer reveals a situation unforeseen by theoretical studies. Such caution is very proper because integral equations are liable to have an embarrassing number of unwanted solutions. The range of solutions of the Wiener-Hopf equation is still far from being understood, and (as far as I know) we still lack a satisfactory theoretical treatment of relatively simple equations like (1).*

* *Postscript.* K. Stewartson *Mathematika* 15 (1968) 22-29 has done some further work on this integral equation in connection with the motion of an electrically conducting fluid, occupying the space between two electrodes at different potentials in the presence of a strong magnetic fluid. He also quotes J. S. Harper and D. W. Moore *J. Fluid Mech.* 32 (1968) 367, who have encountered this integral equation in a problem in the theory of spherical drops.

Appendix IV Problems

Solutions (or partial solutions) to the following problems should be sent to the Secretary and Registrar, The Institute of Mathematics and its Applications, Maitland House, Warrior Square, Southend-on-Sea, Essex, England. Names of successful solvers will be published in subsequent issues of the *Bulletin* of the Institute, together with the best solution selected from those received. Solvers, who are students at school or university, may care to state their ages and affiliations. Readers are also encouraged to contribute further problems of their own for publication. Some of the following problems are much harder than others; and not much indication is given of which are which.

Problem 1. Within the confines of the O-level syllabus prove that

$$\left(1+\frac{1}{8n}\right)\sqrt{(\pi_1 n)}<\frac{2.4.6\ .\ .\ .\ (2n-2)\ (2n)}{1.3.5\ .\ .\ .\ (2n-3)\ (2n-1)}<\left(1+\frac{1}{8n}+\frac{1}{128n^2}\right)\sqrt{(\pi_2 n)}$$

for all positive integers n, where $\pi_1 = 3{\cdot}141$ and $\pi_2 = 3{\cdot}142$

Problem 2. Using A-level mathematics, show that the result in Problem 1 remains true with $\pi_1 = \pi_2 = \pi$ where π is the familiar constant associated with a circle.

Problem 3. A polygon P is called a *jigsaw* of another polygon Q, if P can be cut up into a finite number of polygonal pieces which can be shifted around and reassembled to form Q. What is the necessary and sufficient condition that two polygons P and Q shall be jigsaws of each other? What can you say about the three-dimensional analogue to this?

Problem 4. (Not particularly hard if you know a little calculus). The initial orbital data of the communications satellite Pacific 1 (1967 1A) was: Period $= 1{,}436{\cdot}1$ minutes; Perigee $= 22{,}244$ miles and Apogee $= 22{,}257$ miles, both measured from the surface of the earth; and Inclination (to earth's equator) $= 1{\cdot}3$ degrees. Determine the minimum length of uniform rope required for the Indian rope trick, assuming that the Indian weighs as much as $\frac{1}{4}$-mile of rope. (Don't forget that the earth goes round the sun. If your calculus is rather more advanced and you feel inclined to take matters further, try discussing such complications as the tensile strength and elasticity of the rope, the stability of the system, and the ultimate fate of the Indian).

Problem 5. (Based upon an S.M.P. O-level question). You are given a triangle, a pair of compasses, a straight edge, and no other geometrical instruments; and you are required to inscribe a square inside the triangle (i.e. with the vertices of the square on the perimeter of the triangle). You may *only* use the compasses as follows: given two points A and B, you may draw a circle with centre A to pass through B. Thus, without suitable intermediate constructions, you may *not* draw a circle with given centre A and radius BC (where B and C are given points), since the distance between the compass points may alter uncontrollably whenever the *spike* of the compass (as opposed to its pencil point) is lifted from the paper. Nor may you *directly* draw a circle with given centre to touch a given line. Similarly, the straight edge may *only* be used to draw a straight line through two given points. Can you accomplish the required construction without drawing more than, say, 100 circles; and, if so, what is the minimum number of circles required? How is your answer affected if the triangle is drawn on a piece of paper which does not extend beyond the perimeter of the triangle, so that you may not use any construction points strictly outside the triangle?

Problem 6. In the following table

16	21	8	45	0	0	...
37	29	53	45	0	0	...
8	82	8	45	0	0	...
90	74	53	45	0	0	...

the entries in the second column are obtained as differences (ignoring sign) of successive entries in the first column, the first element of the column being regarded as the successor of the last element: thus $21 = 37 - 16$, $29 = 37 - 8$, $82 = 90 - 8$, $74 = 90 - 16$. The third column is obtained from the second column in the same fashion; and so on. Eventually we get columns whose entries are all zero. Is this a particular property of the four numbers in the first column, or will zero columns eventually arise from an arbitrary first column? (Try out a few cases, if you like). Is it possible to find four integers for the first column such that the fifth column is not zero? If so, what about the sixth column, the seventh column, and so on? If the table has only three rows we can get a case where no column consists entirely of zeros;

16	21	8	13	5	2	3	1	0	1	1	0	1	1	...
37	29	21	8	3	5	2	1	1	0	1	1	0	1	...
8	8	13	5	8	3	1	2	1	1	0	1	1	0	...

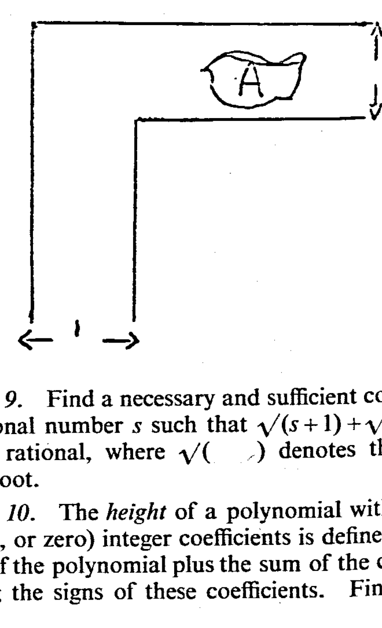
but in this particular table the columns eventually follow a cyclic pattern with three columns to the cycle. Is this true for all three-rowed tables? What happens eventually in a table with m rows?

Problem 7. A cable from Oxford to Cambridge contains n indistinguishable wires ($n > 2$). You are provided with n dark-blue labels numbered $1, 2, \ldots, n$ and with n light-blue labels numbered $1, 2, \ldots, n$; and you are required to attach the dark-blue labels to the Oxford ends of the wires and the light-blue labels to the Cambridge ends of the wires in such a way that each wire carries a pair of labels numbered alike. The allowed procedure is as follows. You start at Oxford, where you may attach the labels as you please and may connect together the Oxford ends of the wires as you please. Then you go to Cambridge and test which pairs of Cambridge ends have been short-circuited by the Oxford inter-connections already made. On the basis of these tests you may then label and connect together the Cambridge ends as you please. Next you return to Oxford, undo the Oxford interconnections and test which pairs of Oxford ends are short-circuited by the Cambridge interconnections. Finally, you may relabel the Oxford ends as you please. No further action is allowed. How do you proceed?

Problem 8. A long passage of unit width has a right-angled bend in it. A flat rigid plate of area A (always kept flat on the floor) can be manoeuvred from one end of the passage to the other. Prove that $A < 2\sqrt{2}$. Show also that, if the plate has a suitable shape (to be determined), we may have

$$A = \frac{\pi}{2} + \frac{2}{\pi}.$$

Is this the largest possible value for A?



Problem 9. Find a necessary and sufficient condition on the rational number s such that $\sqrt{(s+1)} + \sqrt{(s-1)}$ shall also be rational, where $\sqrt{(\ \ )}$ denotes the positive square root.

Problem 10. The *height* of a polynomial with (positive, negative, or zero) integer coefficients is defined to be the degree of the polynomial plus the sum of the coefficients, ignoring the signs of these coefficients. Find a simple

ormula for the total number of distinct polynomials with integer coefficients) having a given height h.

Problem 11. The real numbers $a_1, a_2, \ldots, a_{2n}$ all lie between 1 and 10; and c is the coefficient of x^n in the polynomial expansion of $(x^2+a_1x+a_2)$ $(x^2+a_3x+a_4)$ $\ldots(x^2+a_{2n-1}x+a_{2n})$. You are required to estimate the numerical value of $(1/n)$ log c on the basis of a sample of v pairs (a_{2i-1}, a_{2i}) drawn at random from the set $a_1, a_2, \ldots, a_{2n}$. How do you proceed when $n=10^{10}$, and how large should v be if your estimate is to be accurate to about 1%?

Problem 12. A is a commutative group under addition (with identity 0) and a semigroup (not necessarily commutative) under multiplication. The non-zero elements of A form a group under multiplication (with identity 1); and the distributive law $a(b+c)=ab+ac$ holds for all a, b, c in A. If $-a$ denotes the additive inverse of a for each a in A, prove that $(-1)a=-a$ for all a in A. Show also that, if $(-1)a \neq a(-1)$ for at least one a in A, then A is uniquely determined and the other distributive law $(b+c)a=ba+ca$ is false if and only if $a=1$. Construct (to within an isomorphism) all groups of order 8 and 9; and hence, or otherwise, investigate whether or not A need be a field if it is finite and if $(-1)a=a(-1)$ for all a in A.

Problem 13. Let $\{f_n\}$ and $\{g_n\}$ be sequences of non-negative real numbers such that, for all positive integers m and n,

$$f_{m+n} \leqslant f_m+f_n+g_{m+n} \text{ and } g_n \leqslant g_{n+1}.$$

Prove that f_n/n tends to a finite limit as $n \to \infty$ provided that $\Sigma_{n=1}^{\infty} g_n/n^2$ converges. Prove conversely that, if $\Sigma_{n=1}^{\infty} g_n/n^2$ diverges, there exists a sequence $\{f_n\}$ such that f_n/n does not tend to a limit (finite or infinite) as $n \to \infty$. Can the condition $g_n \leqslant g_{n+1}$ be removed or somewhat relaxed?

Problem 14. Let X denote the set of all numbers which can be expressed in the form $\Sigma_{i=1}^{\infty} 1/n_i!$, where $n_1 < n_2 < \ldots$ is an increasing sequence of positive integers. Let $f(x)$ be a given function which is continuous at all rational values of x. Prove that to each rational number v there corresponds an uncountable subset $X_v \subseteq X$ such that $f(x)$ is continous at $x=v+\xi$ for all $\xi \in X_v$.

Problem 15. A set of points in the Euclidean plane is *convex* if, whenever P and Q are points of the set, all points lying between P and Q on the straight line PQ also belong to the set. A set is *central* if there is some fixed point O in the plane such that, for any pair of points with O as their mid-point, one point of the pair belongs to the set whenever the other does. Prove (preferably by means of an explicit construction) that there exists a largest real number k such that every convex plane set of area A contains a central subset of area kA; and calculate the numerical value of k.

Problem 16. Let a and b be positive integers; and let $f_{a,b}$ denote the number of different ways of packing ab indistinguishable non-overlapping rectangles, each with sides of length 1 and 2, into a rectangle with sides of length a and $2b$. Prove that, as a and b tend to infinity in any manner whatsoever, $(f_{a,b})^{1/2ab}$ tends to a limit. Show further that this limit is equal to $e^{G/\pi}$, where $G=1-\frac{1}{9}+\frac{1}{25}-\frac{1}{49}+\ldots$ is Catalan's constant. What can you say about the corresponding three-dimensional problem of packing abc rectangular bricks (each with sides 1, 1, 2) into a rectangular box with sides a, b, $2c$?

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